...AL LIBRARY
MAR 24 1920
UNIV. OF MICH.

SCIENCE

NEW SERIES
VOL. LI, NO. 1316

FRIDAY, MARCH 19, 1920

SINGLE COPIES, 15 CTS.
ANNUAL SUBSCRIPTION, $6.00

The Royal Road to the Mind

THERE may be "no royal road to learning" but the path through the eye is certainly shorter. Students reason first with their sight; their judgment is actuated by the visual powers.

Just consider, then, how invaluable the BAUSCH & LOMB BALOPTICON is in classroom, laboratory and auditorium. Models are made for showing either lantern slides or opaque objects (photo prints, postcards, specimens, etc.) or both.

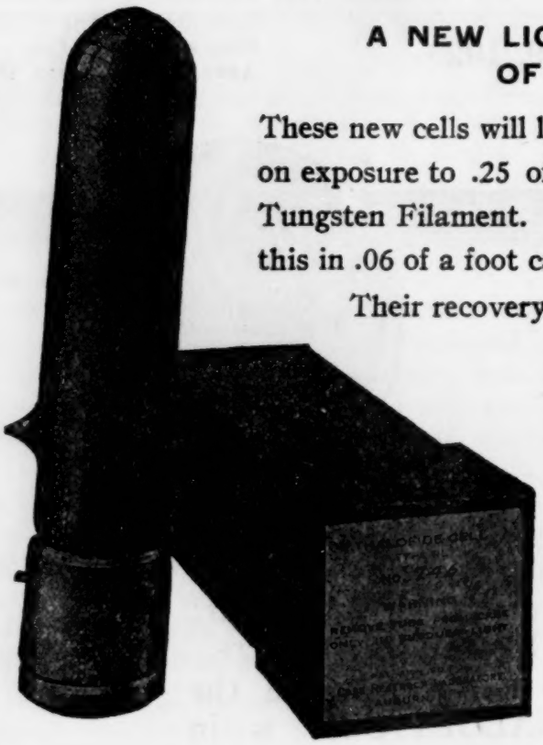
The Balopticon "speeds up" impression making and makes it everlasting. The name of the maker is your guarantee of long thorough service.

BAUSCH & LOMB OPTICAL CO.

New York Washington San Francisco
Chicago ROCHESTER, N. Y. London

Leading American Makers of Photographic Lenses, Microscopes, Projection Apparatus (Balopticons), Ophthalmic Lenses and Instruments, Photomicrographic Apparatus, Range Finders and Gun Sights for Army and Navy, Searchlight Reflectors, Stereo-Prism Binoculars, Magnifiers and other High-Grade Optical Products.

"THALOFIDE CELL"



A NEW LIGHT REACTIVE RESISTANCE OF GREAT SENSITIVITY

These new cells will lower their electrical resistance 50 per cent. on exposure to .25 of a foot candle when the source used is a Tungsten Filament. Special cells are constructed which will do this in .06 of a foot candle.

Their recovery after exposure is extremely rapid as also is their response to the light stimulus which is in marked contrast to the Selenium Cell, especially on exposure to very low light intensities.

Write for Particulars

Case Research Laboratory

AUBURN, NEW YORK

SPENCER SCIENTIFIC INSTRUMENTS

WHETHER

MICROSCOPES, MICROTOMES, DELINEASCOPES, REFRACTOMETERS OR OTHER SCIENTIFIC APPARATUS

Spencer Delineascope Model O, with 400 watt Mazda lamp, 2½ inch objective, Complete.... **$70.00**

MAKE AN INSTINCTIVE APPEAL TO THE LABORATORY WORKER

It isn't only their accuracy and utility. It's something in the finish, even more in the design, but in addition to all these, it is those little things—clever little devices, which accomplish the same end but in a better way—exactly the way that the laboratory worker wants them.

These are the distinctive features of Spencer instruments, made distinctive because our designers, experienced laboratory workers, possess the laboratory view point.

SPENCER DELINEASCOPES

for lantern slides or opaque projection or both have incorporated in them the **Spencer Transposer,** a patented mechanical means for handling the slides, doing away with the old antequated slide holder. It gives a dissolving view on the screen, which is only attained in other outfits by using two Stereopticons, one above the other. ***One Spencer Delineascope does it***

SPENCER BUFFALO U.S.A.

SPENCER LENS COMPANY

BUFFALO, N. Y.

SPENCER BUFFALO U.S.A.

SCIENCE

FRIDAY, MARCH 19, 1920

CONTENTS

Constructive Scientific Research by Cooperation: PROFESSOR BURTON E. LIVINGSTON... 277

Suggestions for Ecologic Investigations in Vertebrate Zoology: WALTER P. TAYLOR... 283

The Attainment of High Levels in the Atmosphere: PROFESSOR ALEXANDER MCADIE. 287

The Separation of the Element Chlorine into Chlorine and Meta-chlorine: PROFESSOR WILLIAM D. HARKINS 289

Wilhelm Pfeffer: G. J. P. 291

Scientific Events:—

The Organization of Scientific Work in India; Portland Cement in 1919; The Investigation of Fatigue Phenomena in Metals ... 292

Scientific Notes and News 294

University and Educational News 295

Discussion and Correspondence:—

Ionization and Radiation: PROFESSOR H. M. DADOURIAN. *How did Darwin work?* PROFESSOR T. D. A. COCKERELL. *A Convenient Demonstration Mounting for Jellyfishes:* N. M. GRIER 296

Organization of the American Geophysical Union: DR. HARRY O. WOOD 297

Special Articles:—

Is Unpalatable Food Properly digested? RALPH C. HOLDER, CLARENCE A. SMITH, PHILIP B. HAWK 299

The Western Society of Naturalists: JOHN F. BOVARD 299

The American Mathematical Society: PROFESSOR F. N. COLE 300

MSS. intended for publication and books, etc., intended for review should be sent to The Editor of Science, Garrison-on-Hudson, N. Y.

CONSTRUCTIVE SCIENTIFIC RESEARCH BY COOPERATION[1]

IT has been occasionally suggested that one of the reasons for the slow advance of science lies in the fact that scientific research problems are still generally attacked by individuals or by small, local groups of workers influenced by a single individual, rather than by planned cooperation among a number of workers in different institutions. Individualistic research has been characterized, by the late Professor C. E. Bessey, as a kind of guerilla warfare upon the unknown. As in other lines of human activity, it seems highly desirable to outgrow this kind of attack, just as rapidly as the appeal of well-planned campaigns and the desire for a maximum of service to race advancement makes itself felt by scientific workers. Commercial research is now frequently carried on in this way, different individuals being actually paid for studying certain aspects of a broad problem and for bringing their minds to bear upon it in a cooperative way. The more fundamental aspects of scientific investigation and the clearing up of the broader, general principles of science have not usually been approached in this manner; the extremely individualistic methods of the Middle Ages seem still to be in vogue.

This state of affairs in science is sometimes thought to be due to the supposed fact that an investigator can not confine himself to what he starts out to study, but that he is constrained, by the nature of investigation itself, to follow his evanescent interests and caprices wherever they may lead. But the scientific researches undertaken and carried out by large commercial establishments and also, especially, those that were so remarkable furthered by preliminary planning and a division of work,

[1] Prepared by request of the chairman of the Division of Biology and Agriculture of the National Research Council.

under the governmental auspices of the nations recently at war, seem to furnish convincing argument against this view. When workers have been paid for solving a specific problem that fits itself into a general scheme of scientific progress—whether payment has been in money or in the approval of their contemporaries or in their own satisfaction in worthy service—it appears that they have been able to perform their separate parts of a broad plan and that they have often accomplished the almost impossible. One prime reason for the medieval methods followed by fundamental research seems to be that significant money rewards are not generally offered for this sort of work and that popular approval still goes to the guerilla rather than to the unit of an organization. The motive of worthy and constructive service to nation and race seems often to be thrust into the background, excepting in so far as the "bias of happy exercise"—with the satisfaction of doing just what one likes to do from day to day, unhampered by previously made plans—gives a person to feel that his own activities must be greatly worthy and outstandingly constructive. It seems, however, that all of us are strongly moved at times by the idea of communal service; for the most part we are glad to consider ourselves as doing our best to be worthy parts of a worthy whole, and therein lies the substratum out of which the morale of an army's research division or of an industrial laboratory is organized.

The cooperative instinct is strong in most scientists, as in other people, but the practical lack of means whereby constructive cooperation might be planned and arranged makes it very difficult indeed for any single worker to break away from medieval individualism in research. To cooperate, it is necessary to find several others with whom to plan and work and with whom the detailed operations may be divided and shared; in short, to organize a cooperation or to find one already organized. If an individual feels the impulse for cooperative service so strongly as to lead him to act, he must first give up his actual investigations for a time, until the needed organization may be created; he does not generally find it in existence. He may approach his colleagues in either of two ways, both of which are apt to lead to bitter disappointment, as things are today. First, he may seek workers who will join with him in attacking a fundamental problem already partly planned in his own mind. Without some money to expend on the work, or some position to offer, he may be met with the suspicion that his motives are ordinarily selfish, that he desires someone else to "pull his chestnuts from the fire." Second, he may offer his services to those who have plans for constructive research problems in mind, but here also he may often be suspected of ulterior motives of low-grade selfishness, and his offers may be responded to by increased secretiveness, so that he may not receive the encouragement he sought. The whole idea of cooperation in such things is so novel that to propose it without money payment seems almost to argue an idealism that verges toward insanity. And yet the conception of cooperation among human beings forms the central strand about which has been braided the cable of most forms of human faith.

It appears that the democratic cooperation that is obviously needed requires an organization that shall not depend on the autocratic leadership of some individual enthusiast, who might soon get looked upon by his followers as a "super-man" or as possessing some sort of divine right (even though he himself might lay no claim to such attributes) and against whom opposition might grow ever stronger because of this very fact. A suitable motto for the organization needed might be that of the French republic, there being as much liberty and equality as is possible with true fraternity (which is cooperation). If such organizations as are here suggested are to be formed they should be cooperative from the very start and should center around several individuals. They should be democratic in nature. It must be clearly understood that the original group have merely tried to plan the work so as to bring about the greatest advancement, and that the original plan is but a temporary scaffold, to be modified from time to time and

finally replaced by other plans, as the work progresses and as new ideas are brought to light. This feature apparently requires repeated emphasis, to offset the mistaken thought that any free thinker is to be hampered in his scientific progress if he joins in with a group of others who already have in mind a broad, although always tentative, plan. On the other hand, it is especially essential that any organization for cooperation should exhibit a strong *esprit du corps*, and if a prospective cooperator feels that his entrance into the organization may result in too great modification of his own ideas of what he should do, if he does not respond to the general aims and motives of the group, he should consider carefully before he joins.

Given the needed organization, safeguarded against autocracy or bureaucracy, around which and in which cooperative research might develop—more as a coagulation due to the internal conditions of an emulsion than as a precipitate forced by a reagent from without—it really seems possible for beginnings to be made, even without considerable financial support. According to the writer's experience with his colleagues it is not true that scientific research workers do not generally wish to cooperate. A number of able workers can be found who will gladly join hands in the prosecution of almost any problem that may be mentioned. It is of course not to be expected that all workers will unite on any particular portion of the vast realm of science; if the project in question is concrete enough to be ready for actual attack there will, of necessity, be only relatively few who will take part. Furthermore, the more fundamental is the nature of the problem, the fewer will be the number of possible cooperators; many would join together to find ways of reducing the cost of living, while only a few could be found to work conjointly on the ultimate nature of life itself! Clearly, the function of the original organizations for cooperative scientific research must be to find the cooperators and to prepare a way by which these may cooperate. To accomplish this, the preliminary organization will of course require time and thought from several persons, and some funds must be available for assistance and for travel. Scientists are not generally able to command even such small funds as will be needed; they will rightly feel that, if they devote time and serious thought to this matter of organization (thus temporarily setting aside their own investigations), the small amounts of money needed should come from elsewhere. It is not necessary, however, to pay for the time and thought of the cooperators themselves, these may be had for the asking; but mechanical and clerical assistance must be furnished to the preliminary organizations if they are to be successful. Without funds for this (and for travel, also, in many cases) such organizations ought not to be started, for without such funds they can do little more than distract the attention of their members from their own researches. An active guerilla warfare seems much better than mere social gatherings that would be unable to act upon a decision even if they should reach one, as to what is needed and what ought to be done. This consideration assumes special importance when it is remembered that the individualistic and non-cooperative method, poor as it confessedly is, is the only one that has been really tested in fundamental research, and that discussions within groups that are without adequate power to act are apt to detract from the volume of individual research, while they may add but little to true accomplishment.

To determine whether cooperators may be found for a given project it seems desirable to begin the organization in an experimental way. The preliminary organization will need widespread publicity among the proper public. Tentative plans for the problem in hand will need to be submitted to many minds, will need to be repeatedly modified or remade, until a sufficient group of workers are willing to enter into the proposed cooperation. Diversity of geographical location and of temperament and interest among scientists, make the preliminary testing of any cooperative project an operation that must necessarily consume much time; several years may generally elapse before a true decision can be reached as to

whether the project itself is really fitted for cooperation. It is clear that men who are busy with other matters will not generally be able to perform this sort of preliminary service unless adequate funds for assistance and travel are available.

In the preceding paragraphs the need and the apparent possibility for cooperation in productive fundamental research has been emphasized, but it is not to be forgotten that there are other lines of cooperative endeavor to which the attention of scientists might turn, lines along which larger numbers of cooperators might be willing to unite. Practical research, for example, which always holds forth hopes of financial return, is more generally attractive than fundamental research. Applied science readily finds financial support, either from individuals or from commercial organizations, while fundamental science is not so generally and practically appreciated. Another field of cooperation in which large numbers of scientists would surely cooperate is that in which lies the problem of suitable publication and dissemination of the results of research. This field also commands considerable support, partly on account of the fact that publications frequently pay their way in the commercial sense and partly because research institutions of various kinds (especially governmental ones) may hope to gain prestige through the publication and distribution, of good scientific contributions. Still another example may be mentioned, in the field of bibliography, with which the writer has dealt to some degree in other places.[2] A great cooperation of this kind, involving hundreds of workers, has recently been inaugurated in the new botanical abstract journal. Finally, there are possibilities for valuable cooperation in making the facilities for experimental research available to more workers than is now the case. Thus a number of workers might unite to find the needed buildings and apparatus for a research laboratory in which any scientist might work; this has been done in several instances. These and many other lines of cooperative endeavor that might be mentioned are all surely worthy of the best that we find in us, and it is not the intention of the writer to maintain that constructive, fundamental research is more important than any other line. This paper, however, is planned to deal especially with cooperation in fundamental scientific research itself, in the solving of the actual problems of science, and so other fields for cooperation are not here dwelt upon. The following paragraphs will set forth some of the apparently possible ways by which the organization of actual cooperative research might be attempted under the auspices of the National Research Council; there is no doubt that such activity lies within the prerogative of the council or of any of its divisions, should they see fit to undertake it.

[2] See Science, 49: 199–207. 1919. The remark in the text refers also to some unpublished memoranda presented to the Division of Biology and Agriculture of the National Research Council.

Possible Organizations for Cooperative Projects

Since actual research deals with somewhat definite and concrete things, it is clear that a separate organization is required for each project and that a project must be relatively narrow in order to be suitable. A committee on the general subject of cooperation in research, without a specific problem, might be valuable in other ways—mainly educational—but it could not further research cooperation unless it narrowed its purview. General committees might be formed (some are already in existence) to canvass the various scientific fields and prepare, from time to time, lists of projects that seem promising for cooperation.

Before it can be decided whether or not a given project for research cooperation is to be undertaken, that project must of course be presented in a rather complete, though preliminary, way. Scientists might be encouraged to present plans for projects. These plans should show clearly what sort of work is contemplated, how it may be divided up among the cooperators, how the results may be handled so as to bring them into the permanent structure of fundamental science, what funds will be required for

assistance, travel, etc., and such other essential features as might enable an executive committee, or other similar board, to make a rational decision. After a set of preliminary plans had received the approval of a division of the council, the most promising course to follow might be to establish a special preliminary committee on the given project. This committee might be instructed to proceed to get the work started if funds are already available, or to attempt to procure these if they are not. It can not be too strongly emphasized that some funds are necessary, even for the activities of such a preliminary committee, for it is neither safe nor desirable to ask research workers to donate money, as well as time and energy, to this sort of endeavor. After the needed funds have become available the preliminary committee may proceed to consult or correspond with all probable cooperators, asking first their aid in completing and elaborating the details of the preliminary plan in such a way that it may be feasible. As this work goes on it should gradually become apparent how many cooperators may be hoped for, and when the preliminary committee judges that the project has reached a feasible stage the committee may enlarge itself so as to include all those of its correspondents who are willing to cooperate. This enlarged committee (which would be the organization mentioned above, as needed before a cooperative project may be actually started) may then reconsider the detailed plan and divide the work up among its members. A project may fail at any stage, even after the enlarged committee has been formed, but it seems probable that a good measure of success may be regarded as fairly assured when this stage shall have been reached. Haste is not desirable, to do good work much time must be allowed, but the preliminary committee would report the project as impossible at present, if it were found impracticable to obtain a reasonable number of cooperators, or the necessary funds for its work.

A cooperative organization started in some such way as this would almost surely be successful, but the contemplated measure of its success must not be too large. It must be remembered that this sort of cooperation, if begun, would tread on new ground and would surely encounter unexpected difficulties. No considerable concrete results need be looked for at the end of a single year and the financial support available at the start ought to give promise of remaining available for several years at least. Nevertheless, the very idea of such cooperative endeavor in research fields is so extremely novel that much discussion and publicity in the proper circles would be needed before it might be realized, and each preliminary plan submitted, each preliminary committee appointed and each letter or publication or conference produced by such a committee, would help to build up the spirit of cooperation. It should be recognized that the fact of cooperation itself is vastly more important than cooperation on any special project; if one project should fail others should be attempted, the work must be regarded as experimental. It would make little difference just what particular problems were undertaken in this way, but it seems highly desirable that *some* problems might be so attacked. Once applied in a concrete case or two, the general idea would surely spread more rapidly than ever could be the case if it were held indefinitely in the phase of *a priori* discussion. As in the prosecution of experimental research itself, it is only by actual trials that it can be found out what degree of success might attend such cooperative organizations as are here suggested.

SOME SPECIAL FEATURES OF COOPERATION IN EXPERIMENTAL RESEARCH

Several features of cooperative research have been impressed on the writer during a number of years' experience with this sort of attempt. First, there appear to be a large number of good experimenters who do not have well-selected problems in mind, who work on that which lies close to them rather than on that which seems to be most fundamental, most far-reaching or most imperatively needed for the growing structure of knowledge. These workers are generally the younger men, and they

almost always prove to be glad to join a cooperation that appeals to them as well planned. Of course they can not usually afford to finance such work and they are not always able to obtain financial support from their institutions, but they are generally very willing to work with great enthusiasm on a cooperative project if the actual expenses can be met. For example, about forty workers joined heartily in an experimental study of evaporation in the United States in 1907 and 1908, and all that was needed was that the requisite apparatus and materials should be furnished, together with postage for their reports. Similarly, about eight persons, none of them professional scientists, cooperated very successfully in an experimental study of the climatic conditions of Maryland in 1914. In this case all apparatus was furnished, and each station was visited fortnightly by the scientist who had the work in hand. For the most part, those who have been cooperating in the project on the Salt Requirements of Plants (Division of Biology and Agriculture, National Research Council) have provided their own apparatus, but it has been necessary to supply some equipment in a few cases and to furnish report blanks, seed, etc. Experience seems to indicate that many people are glad to cooperate if a project is well presented and if it has promise of being continued long enough to produce results. In the cooperations with which the writer has previously had to do, a definite time limit was set from the start and enthusiastic cooperation lasted through the period; indeed, in one case the work was continued by many cooperators for more than a year after the agreement came to an end, but it was possible to find the small amount of needed funds for this extension from sources other than the original one.

To maintain enthusiasm among a group of cooperators it would of course be necessary for the committee to see to it that an active correspondence should be kept up. Every one appreciates being written to about his work, being told of what others of the organization are accomplishing, being able to ask for suggestions and advice when difficulties arise, etc. This means that a central office for each cooperative organization should be maintained, and that some competent person should act as secretary and custodian of records. Here is the main limit placed, by the nature of the work, upon the extent of cooperation in actual, concrete research. The number of cooperators would be limited by the amount of time and the amount of assistance that were available to the person acting as secretary. For obvious reasons formal letters would not be satisfactory, and each cooperator should be treated individually. In actual experimentation of an intricate kind it appears that a single individual, with adequate assistance, can care in this way for not more than perhaps a dozen cooperators. With a much larger group the work of the central office would have to be divided and the personal nature of the correspondence would be largely sacrificed. Of course a larger cooperative organization might be arranged in sections, each with its central office, but where all the work interlocks intimately with all the other work such subdivision would probably introduce difficulties requiring much special study. Furthermore, the experimental results obtained by the various cooperators would require very thorough study, tabulation and presentation in other ways, before they might take their place in the planned whole. This work can not generally be accomplished by the cooperators themselves, though each worker would always make his own interpretations as he proceeds. The central office would carry on the work of correlation and would keep the cooperators informed as to new developments coming from the work of others and from combinations of results from several sources. This feature of productive cooperation in research also sets a limit to the number of cooperators that may profitably work together on a concrete problem. Finally, it needs to be emphasized that the work as a whole would require adequate presentation in some suitable form of publication and that individual publications of the cooperators—although these might form a basis for this treatment of the whole problem—would not

suffice. What seems most needed are constructive and progressive contributions toward the solution of definite problems that are ready for experimental attack, and the central office of such an organization as is here considered would plan to undertake these. Preliminary presentations might be prepared and submitted to all cooperators. Out of the correspondence thus developed would eventually come a presentation that might measurably approach a truthful one, whereas single individuals could not hope to do more than make incomplete and more or less one-sided contributions in the desired direction, their papers being similar to most of those that now appear in the scientific publications.

From the last paragraph it will be appreciated that the writer's idea of cooperation in research involves the union of a number of minds in planning the attack on a problem, in working out the different parts, and in bringing the several component results together into a well-considered presentation that might really mark a tangible advance in scientific knowledge. Cooperations of this sort would bring it about that many of the experimental mistakes that cause so much discussion in scientific literature might be avoided at the start (through cooperative planning) and that most of the adverse criticism that leads to such wasteful polemics in many scientific fields might be already past before the main publication occurred, for each cooperator—and perhaps others also—would act as critic regarding the general presentation while it was still in manuscript form.

BURTON E. LIVINGSTON
THE JOHNS HOPKINS UNIVERSITY

SUGGESTIONS FOR ECOLOGIC INVESTIGATIONS IN VERTEBRATE ZOOLOGY[1]

IN a recent message transmitted to local administrators throughout the country praising them for their efforts during the war Food Administrator Hoover declared that the American people now "are summoned to a still larger task—to provision the Allies and the liberated nations of Europe, which face not their civilization together unless a steady stream of food supplies can be kept flowing to hunger alone, but the collapse of all that holds them to repair their gravest deficiencies, and in far greater volume than by utmost stress was sent last year."

As is well known to everyone, under the continuous and effective stimulus of the United States Department of Agriculture there has taken place a speeding-up process on the farms throughout the nation, a process which must apparently be continued and even augmented if we are to succeed in our wrestlings with the problem of world food shortage.

It is obvious that one very practical way in which to increase food production is to cut down the losses due to plant or animal pests. The department has addressed itself with extraordinary vigor to this problem and a comprehensive program in pest control is being administered by the different bureaus. That portion of the program concerned with reduction of losses due to rodents and other mammalian or bird pests devolves upon the Bureau of Biological Survey.

Current estimates place damage done to the carrying capacity of the open range and to cultivated crops generally by rodents in the western states at $300,000,000 annually. Add to this the destruction of live stock by predatory mammals, estimated at some $20,000,000 every year, and the damage done to goods in warehouses and stores throughout the United States by rats and mice, an additional $200,000,000, and we have an impressive total. Particular interest attaches to these figures at this time in view of the comprehensive plans for the reclamation of arid and other lands in behalf of returned soldiers recommended by the Secretary of the Interior and given favorable mention by the President of the United States in his latest address to Congress. Potential or actual rodent pests exist on nearly every acre of the arid land which it is proposed to reclaim. In some sections effective

[1] Read before the Ecological Society of America, Johns Hopkins University, Baltimore, December 28, 1918.

rodent control will be an absolute prerequisite to successful dry land agriculture.

Important as this side of the work undoubtedly is, the destructive or pest-control aspect of the work is not the only one to which attention is given. Constructive measures under consideration include such items as game protection, with its multitudinous perplexities, biological and legislative; further domestications of wild species, as ducks and other game birds, deer, elk, buffalo and fur-bearing mammals; possible use for food of available wild animals not now so utilized; artificial stimulation and increase of beneficial wild species of birds and mammals; introduction and acclimatization of birds and mammals; administration of bird and mammalian resources of zoological parks, national forests, game preserves, bird reservations and national parks.

It will be readily realized that problems of extreme difficulty arise in connection with both the program of pest eradication and that of the development and increase of beneficial species, problems the solution of which depends upon a much more adequate knowledge of and control of the balance of nature than man has yet been able to acquire.

Years ago Spencer F. Baird called attention to the fact that the only rational basis for fisheries administration is the complete knowledge of aquatic creatures to be acquired by intimate investigation. He emphasized the fact that it would be of doubtful value to study only the major forms which supported fisheries and that "useful conclusions must needs rest upon a broad foundation of investigations purely scientific in character." This generalization applies with at least as much force to the terrestrial vertebrate fauna as it does to fishes. With this thought in mind, the Biological Survey has for more than thirty years been carrying forward investigations in North America dealing primarily with the geographical distribution and habits of birds and mammals.

It seems to be clear that this work should not only be continued but should be expanded; and that expansion may well take place in the direction of a more intensive investigation of the relation of animal to environment on a dynamic as well as static basis.

The expansion of ecologic activities by the Biological Survey and other agencies engaged in biologic researches does not, of course, exclude the prosecution of the faunal natural history investigations. On the other hand work in ecology implies a fairly thorough knowledge of faunas and floras. It is not unlikely that new methods of the ecological type may be utilized profitably in connection with natural history surveys. In addition to this the ecologic point of view should be tried out in the interpretation of distributional data.

As compared with the plant ecologist, the animal ecologist is working at a considerable advantage. Though many problems of method and approach peculiar to the animal side of the work are bound to present themselves, much of the way over which the animal ecologist must go has been traversed already by his botanical colleague; and by virtue of this fact it should be possible for him to avoid many missteps and false leads. Instrumental studies of the environment have been carried forward to a very considerable extent by the botanist, thus relieving the zoologist of some, at least, of this fundamental labor. Furthermore, many of the guiding principles for research work in plant ecology, already enunciated by the botanist, can be adapted to the field of animal ecology.

Of course, with animals, the factor of mentality introduces a host of new problems into the study which are almost wholly lacking with plants. To a certain extent the new difficulties resulting from this factor neutralize the advantages derived from the fact that the botanists have done pioneer work.

The comprehensive demands of the ecologic program peculiarly emphasize the desirability of cooperative effort. The association of individual investigators who are specialists in botany, chemistry, geology and meteorology, as well as in zoology, will often be necessary to the adequate organization of the work; and it may often be advantageous for institutions as well as individuals to work together. It is

obvious that many universities and colleges, by virtue of their locations and resources, have abundant opportunity to perform valuable work in the ecology of vertebrates.

Although for some of the more detailed researches in the physiology, chemistry, habitat relations and psychology of animals a considerable amount of apparatus is necessary, investigations of the highest importance may be carried forward with relatively simple equipment. The field is sufficiently comprehensive to afford promising opportunities to any sincere and resourceful student with proper training.

Suggestions as to equipment needed in the different lines of work indicate the wide latitude of choice open to the prospective investigator. For the prosecution of field work there should be provided camp outfit, traps and collecting materials, photographic apparatus, balances and if possible, equipment for field study of habits, such as shovel, axe, trowel or large spoon, brush cutter, tape-line, sketch pad, coordinate paper, and writing materials. For more detailed study of the habitat apparatus such as thermometers and thermographs, wet-bulb thermometers and psychrographs, rain gauges, geotomes, etc., are required. Investigations of the chemistry and physiology of the animals in question call, of course, for special equipment; and in connection with many of the studies it is necessary to provide some cages, pens, yards or other enclosures for breeding and keeping terrestrial vertebrates under close observation.

The richness and attractiveness of the field may be amply demonstrated through the simple device of a tentative program of work.[2] It is fully realized, I may add, that a program of research may be outlined with comparative ease, but that it is often relatively difficult to get results. But the drawing up of this program, embracing, as it does, material for many investigations, is doubtless justified in that it indicates the immensity of the field, implies the necessity for widespread cooperation in the exploration of its resources, and points out some comparatively neglected fields of research in vertebrate zoology which are full of promise.

I. Close analysis of the animal community.
  1. Community reactions.
    (*a*) Origin.
    (*b*) Migration.
    (*c*) Invasion and reinvasion.
    (*d*) Establishment.
    (*e*) Competition.
    (*f*) Dominance.
    (*g*) Extinction.
  2. Primary and secondary succession in the animal community.
    (*a*) Primary succession as illustrated by zonation in selected localities.
      (1) In deserts.
      (2) In areas of moderate humidity.
      (3) In regions of excessive humidity.
    (*b*) Secondary succession as illustrated by results of interference with the natural balance by man.
      (1) Permanent changes in animal population.
      (2) Reinvasions of abandoned areas.
      (3) The animal assemblages of over-grazed areas; of areas denuded by deforestation; of forest burns; of flooded areas; of drained areas; of reclaimed areas in general.
    (*c*) Climax assemblages of animals.
      (1) Significance for game protection.
      (2) Significance in animal control.
    (*d*) Factor control of distribution and succession among animals.
      (1) Physical factors.
      (2) Biotic factors.
  3. The animal community and climatic cycles.
    (1) Interrelations with forest reproduction.

[2] I am indebted to Dr. Frederic E. Clements, of the Carnegie Institution of Washington, for helpful suggestions in this connection.

(2) Interrelations with forage production on the open range.
(3) Interrelations with crop production.
(4) Climate and fluctuations in animal populations.

II. Analysis of relation of individual animal to its environment.
1. Breeding habits.
2. Migration.
3. Hibernation and estivation.
4. Nests, shelters and other structures.
5. Interrelationships of species.
6. Adaptation of particular life forms to the environment.
7. Controlled investigations.
(1) In the laboratory.
(*a*) Behavior.
(*b*) Food.
(*c*) Adaptation and response.
(*d*) Domestication.
(2) In the field.
(*a*) Fenced areas for special study, *e. g.*, of damage done to forage.
(*a*) Rodent inclosures; exclosures.
(*b*) Eradication plots; reinvasion plots.
(*c*) Census and burrow investigation areas.
(*d*) Reseeding plots.
(*b*) Comparison of animal responses in different measured habitats.

III. Analysis of broad movements of animal populations through time.
(1) The paleontologic record.
(2) Present distribution.
(3) Relationships of animals to the envirments of the past.
(4) Successional communities of animals.

IV. Analysis of data of geographical distribution of higher vertebrates.
1. Realms, regions, life zones, faunal areas, formations, associations, animal communities in general.

V. Economic aspects of analysis of the animal community.
1. Animals and products of the farm.
(1) Rodents and crops.
(2) Relation of birds to agriculture.
(3) Predatory animals and the stock industry.
(4) Economics of fur bearing animals.
2. Animals and reforestation.
(1) Effect of rodents on natural or artificial seeding.
(2) Big game and the forest.
(3) Birds and insect tree enemies.
(4) Animals and forest burns.
(5) Animals and logged over areas.
3. Animals as related to the grazing problem.
(1) Effects of rodents on carrying capacity of the range.
(2) Elucidation of the relations of cattle, sheep, goats, big game, predatory animals, rodents and plants in the disturbed conditions now prevailing on the open ranges of the West.
(*a*) Comparison with conditions in northern Africa, Europe, Asia, Australia.
(3) Permanent vegetative changes produced by the unrestricted grazing of cattle and rodents, and their significance from the standpoint of range maintenance and the future maximum productivity of the land.
4. Further domestications of wild species of animals.
(1) Ducks and other game birds.
(2) Deer and elk.
(3) Fur bearing mammals.
5. Statistics of animal economics.
(1) Estimates of numbers of rodents and other mammals and birds of economic significance in different types of country.
(2) Estimates and determinations of extent of different types of country in the United States.
(3) Estimates of benefits or losses conferred by different species of vertebrates.
(*a*) As individual animals.
(*b*) Aggregate for species as a whole.
(4) Estimates of total losses from rodents and other harmful mammals in the United States.

(5) Estimates of cost of complete control of noxious species, together with amount of probable saving that would result.

6. Beneficial animals and their preservation.
   (1) What animals are beneficial?
   (2) Relation of age of species to problem of its preservation.
   (3) Effect of occupation by man on animal community.
   (4) Essentials for conservation.
      (*a*) Maintenance of seed-stock; determination of annual toll permissible; unfair methods of destruction; effect of legislation on game conservation.

7. Noxious animals and their destruction.
   (1) What animals are noxious?
   (2) Methods of control; rodents, predatory animals, other groups.
   (3) Effects of extirpation of wild species on the natural balance.

By way of summary, may I repeat that present day world politics emphasize in unmistakable terms the vital necessity of increase in food production. For permanent increase in the productivity of the land further study is called for of the scientific fundamentals on which agricultural practise is based. The ecologic method of approach promises much of value. The problems are vast and lead inevitably to the stressing of the strategy of cooperation as an essential to their successful solution.[3]

WALTER P. TAYLOR
BIOLOGICAL SURVEY

[3] The symposium on the relations between government and laboratory zoologists held in connection with the meeting of Section F at the sessions of the American Association for the Advancement of Science, Johns Hopkins University, Baltimore, December 28, 1918, emphasized the desirability of better coordination and cooperation between these two large and active bodies of scientific investigators. In this connection the Bureau of Biological Survey solicits correspondence from all who contemplate researches in the ecology of the higher vertebrates, and will be glad to assist with suggestions, advice, or otherwise as opportunity may be given.

THE ATTAINMENT OF HIGH LEVELS IN THE ATMOSPHERE

IT is a far cry from January 7, 1785, to February 27, 1920. On the earlier date Dr. John Jeffries ascending from the cliffs at Dover, made his way through the air over the English Channel to France, landing after an eventful three hours, on the French coast in the forest of Guines.

During the flight his barometer ranged from 1,006 kilobars (29.70 inches) to 789 kilobars (23.30 inches) indicating at the lower reading a height of nearly 2,012 meters (6,600 feet).

On Friday, February 27, this year, Major R. W. Schroeder, chief test pilot of the Air Service, rose from McCook Field at Dayton, Ohio and reached an elevation of 10,979 meters (36,020 feet).

Jeffries of course used a balloon; Schroeder an airplane designed for climbing, and with a supercharger, *i. e.*, a gas turbine centrifugal compresser to offset the loss at the carbureter due to diminished density of the air at such heights.

The history of the attack upon the high levels of the atmosphere may then be said to extend over a period of one hundred and thirty-five years. Various methods and agencies have been employed. Within twenty years from the time of the first ascension, heights of 4,000 meters had been attained. Indeed Gay-Lussac made certain scientific observations at a height of 7,400 meters in 1804.

On September 5, 1862, Glaisher and Coxwell reached a height of 11,200 meters or practically the same level as that reached by Schroeder with an airplane. Three other noteworthy records by manned balloons are those of Tissandier, Spinelli and Sivel, acting for the French Academy, who reached a height of 8,530 meters, on April 15, 1875; Dr. A. Berson who on December 4, 1894, reached 9,600 meters; and later (1901) Berson and Süring to a known elevation of 10,500 meters and probably 10,800 meters, both men being unconscious at the higher level. In all of these high balloon flights, the observers became unconscious, and this even in the

later attempts when recourse to oxygen inhalation was had. In the airplane and Zeppelin ascensions to be referred to later, the observers were provided with oxygen, and what is equally important, body heating devices to enable them to withstand extremely low temperatures.

While not, strictly speaking, a manned balloon, it must be noted that in the famous Zeppelin raid of October 19, 1917, the barographs of the flagship L 49, superdreadnaught, indicated that at least for a short period the airship had attained a height of 6,200 meters. The crew were provided with oxygen tubes and wore electrically heated mittens and boots. There is some doubt, however, concerning the height, owing to the speed of wind and ship.

A brief summation[1] of the extreme elevations attained, up to 1917 is:

By kites, 7,044 meters, Mt. Weather, Va., Oct. 3, 1907.

By manned balloons 10,800 meters, Berson and Süring, July 31, 1901.

By Zeppelin, rigid dirigible, 6,200 meters, October 20, 1917.

By airplane, 7,950 meters, G. Guidi, Nov. 7, 1916.

By sounding balloons, 37,000 meters, 1912.

By pilot balloons, height determined by theodolite, 39,000 meters.

The airplane record has been steadily developed. In 1909 Latham made 161 meters; which was soon surpassed. Drexel in 1910 made 1,829 meters and then in rapid succession Macrane 2,582 meters, Wynmalen 2,800 meters, Drexel 2,880, Johnston 3,193, Loudan 3,280, Parmelee 3,304, Brindley 3,585, and Legagneux, 5,718, a noteworthy jump.

Perreyou on March 11, 1913, attained a height of 6,000 meters.

The war gave a tremendous impetus to the development of the plane; and the necessity of quick and high climbing was fully appreciated by all the belligerents.

Major (then Captain) Schroeder on September 18, 1919, reached a height of 8,809 meters (28,000 feet) at Wilbur Wright Field. Captain Lang and Lieutenant Blowers of the Royal Air Service, in the brief space of 66 minutes, reached an elevation of 9,295 meters (30,500 feet); to be in turn surpassed by Roland Rohlfs at Roosevelt Field, Mineola, who made 9,357 meters (30,700 feet) on July 30, 1919, and again on September 18, of the same year when in the short space of 78 minutes he rose 10,516 meters (34,500 feet) and fluttered back to earth as gently as a snowflake drops.

Mention should also be made of the flights of Adjutant Casales on May 22, 1919, to 9,449 meters, June 8, 9,495 meters, and on June 14, to 10,100 meters (33,136 feet).

The record now stands Schroeder, February 27, 1920, 10,979 meters. Thus in a period of ten years the heavier than air machine has been so improved that elevations have increased from 500 to practically 11,000 meters. We are told that the goal of American aviators is 12,000 meters or approximately 40,000 feet, but it is of course, possible that this ceiling shall be lifted still higher, and that a height of 15 or even 16 kilometers (10 miles) may be reached, provided suitable protection (so-called diving suits) for the airman is available.

In Schroeder's latest ascent, the oxygen supply was exhausted and the results were tragic but fortunately not fatal.

The fact that heavier than air machines can be driven to the 10-km. level means much to the aerographer, particularly in connection with forecasting weather changes at the surface. This is the most important level for studying not only pressure, temperature and water vapor content, but the air flow and structure of cyclone and anticyclone. The 10-kilometer level is the bottom of the stratosphere or isothermal region and at the same time the top of the troposphere or convectional region. As a postulate to Dines's statistical studies we know that in the stratosphere or region above 10 kms. it is colder in an anticyclone than in a cyclone at the same level, while on the other hand in the troposphere, *i. e.*, from 9 kms. down to 1 or 2 kms.

[1] From "Principles of Aerography," p. 19.

it is warmer in the anticyclone than in the low. This holds for Europe but is not entirely confirmed for the United States. The height of the base of the stratosphere varies in Europe with cyclonic and anticyclonic weather from about 8 to 13 kms. It also varies with latitude, averaging 9.6 at Petrograd; 10.6 in England; 11 in Italy; and 11.7 in Canada.

Thus it can readily be predicted that at a height of 10 kms. in the latitude of New York an airman rising on an afternoon in the early fall will experience a temperature lapse or vertical decrease amounting in all to 200 kilograds, *i. e.*, from 1,050 to 850 kilograds, using a scale on which the absence of all molecular heat is represented by 0 and the ordinary freezing point by 1,000. On the Centigrade scale this would be from 14° above freezing to 41 degrees below freezing.

If our atmosphere were homogeneous, we should reach its top at a height of 8,000 meters. There would then be no need of superchargers; and oxygen tanks would be advantageous but not absolutely indispensable. But this does not occur in nature and the density of our aerial envelope at 8,000 meters is actually 40 per cent. of what it is at the surface. At 10,000 meters it is just 33 per cent. of the surface density.

Km.	Kk.	Kb.	gm/m^3.
20	783	55	87
19	787	63	102
18	783	74	121
17	772	87	144
16	772	102	169
15	772	120	198
14	776	142	233
13	783	167	268
12	790	195	314
11	802	228	365
10	816	266	415
9	838	309	470
8	864	358	528
7	890	413	592
6	920	475	662
5	945	543	733
4	967	618	815
3	989	703	905
2	1,008	798	1,011
1	1,018	903	1,134
0	1,033	1,017	1,258

The preceding table somewhat modified from the data given by Dines in his recent paper on the "Characteristics of the Free Atmosphere" indicates the average temperature, pressure, and density of the air at various heights. The height is in kilometers, temperature in kilograds, pressure in kilobars and density in grams per cubic meter.

Schroeder's thermograph indicated a minimum temperature of —55 degrees C. (or 99 degrees below freezing on the Fahrenheit scale). This on the new temperature scale is 799. It will be seen that this temperature indicates a height of about 11,000 meters.

In one of Rohlf's ascents he went beyond the top of the troposphere or above what might be called the temperature lid. On that date, the base of the stratosphere was below 10 kilometers and, therefore, he passed into a somewhat warmer level even though at a greater elevation.

ALEXANDER MCADIE
BLUE HILL OBSERVATORY,

THE SEPARATION OF THE ELEMENT CHLORINE INTO CHLORINE AND META-CHLORINE

ALTHOUGH many attempts have been made to separate an element into two or more different atomic species, in no case have the experiments met with success. In my opinion this has been due largely to the fact that in all cases where it is *known* that isotopes exist, as in the element lead, the conditions imposed upon the experiments by the relative atomic weights of the different atomic species are such as to be extremely difficult to meet. For this reason, when five years ago I decided to make a separation of an element into isotopes, it seemed that it would be easier to separate the isotopes in an element where isotopes were not known to exist, than to meet the extremely arduous conditions of the known cases.

In 1915 I gave conclusive evidence that chlorine, magnesium, and silicon (in addition to neon as discovered by Thomson), among the light elements, are mixtures of isotopes, and that the atomic weight of the lighter isotope is 35.0 for chlorine, 24.0 for mag-

nesium, and 28.0 for silicon. Among the heavier elements there are probably few elements which are not mixtures of isotopes. Thus there is excellent evidence in the atomic weights that the following elements are mixtures: nickel, copper, zinc, and practically all of the other elements form atomic number 28 to atomic number 80. The radio-elements from thallium (No. 81) to uranium (92) were at that time known to be mixtures of isotopes.

Experiments on the diffusion of chlorine gas were begun by Dr. W. D. Turner and myself in 1916, and early in 1917 slight differences in density were detected, but the chlorine was not entirely pure. Chlorine was used because it could be obtained in cylinders, and its flow was therefore very easy to control. However if chlorine consists of two isotopes, chlorine (Cl) and meta-chlorine (*Cl*), there are three forms of molecular chlorine: Cl-Cl, Cl-*Cl*, and *Cl-Cl*, and this is unfavorable to the diffusion. For this reason we have carried out most of our work by the use of hydrogen chloride gas, which, while unfortunately not obtainable in this country compressed in cylinders, at least has the advantage that its molecules contain only one atom of chlorine each, and that the hydrogen of the molecule has little effect in increasing the molecular weight.

This work was interrupted by the war, but by the summer of 1919 about ten thousand liters of gas had been diffused, a part of this diffusion being done by T. H. Liggett. In October, 1919, I interested Mr. C. E. Broeker in this problem. He has diffused about eight thousand liters of this gas and we hope soon to have an enlarged apparatus capable of diffusing a thousand liters per day, in the first section. At present we have five large units in operation or ready for operation.

The separation by diffusion of gases whose molecular weights lie close together is, according to the diffusion theory of Lord Rayleigh, an extremely slow process. Up to the present time we have concentrated our efforts upon the diffusion itself, and have spent little time in analytical work. The preliminary analyses indicate that the density of the fraction which remains inside the diffusion tubes, is increasing at about the rate predicted by the Rayleigh theory of diffusion, if we consider 35.0 as the atomic weight of chlorine, and 37.0 as the atomic weight of meta-chlorine. We have tested for most of the impurities which might be present except arsenic trichloride. The hydrogen chloride gas is generated from C. P. hydrochloric acid by the action of C. P. sulphuric acid. The next step in our work of proving that a separation has been effected is to secure larger quantities of diffused material, since our final fractions are still small, so that we may be more certain of our purification of the material, and then to make precise atomic weight determinations. If on such further purification we obtain an atomic weight for the heavy fraction as high as that already obtained in our preliminary analyses, we will have definite evidence that we have separated chlorine into a heavier and a lighter isotope. This will be of importance in two ways, first, it will be the *first experimental separation of an element into parts,* and second, it will be one of the strongest links in the proof that *the nucleus of the hydrogen atom is actually the positive electron.*

As stated above, I gave in a series of papers published five years ago[1] a system of atomic structure which gave very strong indications that chlorine, magnesium, silicon, and the other elements specified, are mixtures of isotopes. In fact this system of structure, for which there was much evidence, depended for its validity upon the existence of these isotopes, and in 1916 I published a preliminary notice[2] stating that we were working, on the separation of chlorine into isotopes. It is of great interest that Aston in a preliminary note written to *Nature* in December, 1919, states that his results obtained by positive rays indicate that both chlorine and mercury are mixtures of isotopes, with atomic

[1] *Journal of the American Chemical Society,* 37, 1367–96, especially pages 1390, 1391, 1387.

[2] *Ibid.,* 38, p. 19.

weights 35 and 37 for chlorine, thus confirming to this extent my theory with respect to the light elements and also for the heavy elements. Also in accord with the theory presented in my papers on atomic weights, he finds that the atomic weights on the oxygen basis are practically whole numbers.

The details of our experimental work on the separation of chlorine will be published as soon as we have collected enough material to enable us to make a more careful purification of our material, and when in addition the accurate atomic weight determinations have been completed. We expect to make the final separations by thermal diffusion. The theory of this method has been worked out by Chapman. Mr. Broeker and I are also beginning preparations for an extensive attempt to separate hydrogen into hydrogen and meta-hydrogen, the latter with an atomic weight of 3.0. While there was sufficient evidence for the existence of a meta-chlorine in ordinary chlorine to be found already in the atomic weights, there is no such evidence that ordinary hydrogen contains meta-hydrogen. However, there is evidence that the meta-hydrogen nucleus of a formula $h_3e_2^+$, where h is the hydrogen nucleus and e is the negative electron, is the most important unit in the building of atomic nuclei, with the exception of the alpha particle ($h_4e_2^{++}$). The nucleus of an isotopic atom of higher atomic weight differs from the nucleus of the normal atom by the presence of a mu group (h_2e_2) which carries no *net* charge, and which, if it were alone, would have an atomic number zero. Isotopes of higher atomic weight are also formed by the addition of alpha groups ($h_4e_2^{++}$), each alpha group being attached by two cementing electrons. This is equivalent to the addition of an h_4e_4 group. The details of this system will be found in a paper now in print in the *Physical Review*.

It should have been noted in the above paper that neon, magnesium, and silicon, the even numbered light elements which contain isotopes, lie *adjacent* in the even numbered series, since their numbers are 10, 12, and 14. It is *possible* that a third isotope of chlorine exists.

WILLIAM D. HARKINS
UNIVERSITY OF CHICAGO,
February 28, 1920

WILHELM PFEFFER

WILHELM PFEFFER, with Sachs the founder of plant-physiology as it has been studied for more than a generation, died in Leipzig on January 31, of this year. A long line of Americans, as well as many other foreigners, resorted to him, in addition to the Germans who studied with him. He probably shared with Strasburger the distinction of having a larger number of foreign students of botany than any other German university professor. By these men, and many others, he will be remembered as a striking personality as well as a great leader in the science to which he devoted his life.

The details of his life are probably known to few Americans, but the general outlines may well be set down. He was born March 9, 1845, in Grebenstein near Cassel, the son of an apothecary. The elements of science, and scientific curiosity, he probably acquired from his father; for the old-time German Apotheker was a very different sort of person from the American drug-store proprietor of to-day. He studied at the universities in Göttingen, Marburg, Berlin and Würzburg, taking his doctor's degree at Göttingen in 1865. He began his teaching career as Privatdocent in Marburg, going thence as Ausserordentlicher to Bonn and as Ordentlicher Professor to Basel, Switzerland. In 1878 he removed to Tübingen where, I believe, the first Americans worked with him, Goodale of Harvard, Wilson of Philadelphia, Campbell of Stanford, and perhaps others. In 1887 he went to Leipzig, where he stayed for the rest of his life, in spite of calls to what, to others, might have seemed more attractive posts. But in the Botanisches Institut in Leipzig he had a laboratory fitted to his ideas and desires, with a garden adjacent in which the material which he and his associates used could be readily grown, a garden of such size, position, and plan that it took the

minimum of time for administration. The university and state administrations were able and willing to give him cordial support, and he made his laboratory the resort of all who were pursuing plant-physiological studies or were interested in them.

His long list of publications, beginning with one on mosses, plant-geographical in character, and one or two embryological papers, not only opened the way for plant-physiological work by many others, but directed and to a great extent molded their studies. He not only influenced botanical study, but his osmotical investigations were and are of fundamental importance in physical-chemistry. As friends and neighbors for years, Pfeffer and Ostwald conversed and thought together, to the corresponding advantage of the sciences to which they were devoted.

Two publications stand out from the many because of their general, rather than special, botanical interest, namely the Handbuch der Pflanzenphysiologie, which passed through two editions and in the second was translated by Ewart into English, and was the great reference book in plant-physiology for two generations; and the Jahrbücher für wissenschaftliche Botanik, founded by Pringsheim, and continued after Pringsheim's death and until Strasburger's, in collaboration with him. The Untersuchungen aus dem botanischen Institut zu Tübingen, which lasted only during Pfeffer's stay at the south German university, gave him experience in the mechanical detail as well as in the editorial work of serial publication.

Pfeffer is survived by his widow, his daughter-in-law, and a grandson. He had one child, a son who was of age to be one of the direct sacrifices of the war, and presumably was. But he himself, a man of deep feeling and clear vision, must also have been a sacrifice.

Just as the war began, a jubilee volume was being prepared to celebrate the fiftieth anniversary of his doctorate and his seventieth birthday. Contributions had been promised by his students all over the world. With the coming of war many found themselves prevented from sending their papers, and in consequence the Jubilee Volume which appeared in 1915, as part of the Pringsheim series of Jahrbücher, contained only a fraction of the contributions to science which his students had planned to make in his honor.

Belonging to an age in Germany in which ideas were more sought than discipline, when scholarship was more honored than military rank, when a professor was more likely than a tradesman to become a Geheimrath, his life lasted through the fall of German imperialism and came to an end before German reconstruction was more than begun. Honors, national and international, were conferred upon him; and we who had the privilege of studying under his direction will continue to honor him as an inspiring teacher and a great example of scholarly devotion and productiveness.

G. J. P.

SCIENTIFIC EVENTS

THE ORGANIZATION OF SCIENTIFIC WORK IN INDIA[1]

The reorganization and development of scientific work in India are now under consideration, and important and far-reaching decisions on these questions will shortly be made by the Secretary of State. It has already been decided, both by the government of India and by the Secretary of State, that large sums of money must be found at the earliest possible moment for the purpose of fostering the development of the Indian empire by means of scientific research. The principle of state aid on a generous scale has been accepted, but the important question of the best method of utilizing this form of assistance in the future development of India remains to be settled. These matters were referred to by the Viceroy on January 30 last in his speech opening the present session of the Imperial Legislative Council at Delhi. It is evident from the report of Lord Chelmsford's remarks which appeared in the London *Times* of February 6 that the government of India is now considering large schemes of expansion in regard to the scientific activities of the state.

Two policies at present hold the field: (*a*) Centralization under a proposed Imperial De-

[1] From *Nature*, February 19, 1920.

partment of Industries of the government of India in which chemists, botanists, zoologists, and so on will be formed into distinct, watertight, graded services, each under the control of a departmental head; and (*b*) decentralization under which the scientific workers at the various universities and research institutes will be given as free a hand as possible.

The policy of centralization and the creation of graded scientific services have been strongly advocated by the Indian Industrial Commission, which was presided over by Sir Thomas Holland, formerly director of the Geological Survey of India. It is favored by a number of administrators in India who consider that some measure of official control is necessary for all scientific investigators, and it has also received the support of several of the scientific witnesses examined by the commission. The arguments advanced by Sir Thomas Holland and his supporters in favor of centralized scientific services are set out in detail in Chapter IX. of the Report of the Indian Industrial Commission, published last year.

PORTLAND CEMENT IN 1919

PRELIMINARY estimates compiled by the United States Geological Survey, Department of the Interior, indicate that the production and shipments of Portland cement in 1919 increased 13 and 21 per cent., respectively, over those in 1918 and that the stocks decreased 52 per cent., so that at the end of 1919 less than 5,000,000 barrels of Portland cement was in stock at the mills. The Portland cement industry was set back considerably in 1918, when war restrictions reduced the shipments from the 90,703,474 barrels shipped in 1917 to 70,915,508 barrels, but it is now regaining its lost ground. Early in 1919 the business was even poorer than in 1918, and practically all the increase reported was made during the latter part of 1919, so that 1920 started with favorable prospects for the cement industry. During 1919 the shipments from some mills were limited by the lack of freight cars. The increase in the value of the cement shipped in 1919 over that shipped in 1918 was about 28 per cent.

The shipments of Portland cement in 1919 amounted to 85,485,000 barrels, valued at $144,461,000; the production amounted to 80,287,000 barrels; and the stocks at the mills at the end of the year amounted to 4,976,000 barrels.

One new plant produced Portland cement in 1919—the Indiana Portland Cement Co., at Greencastle, Ind. The total number of plants that produced cement in 1919 was 110, and the total number of plants that shipped cement was 113. The average factory price per barrel for Portland cement in bulk in individual states in 1919 ranged from $1.57 in Kansas to $2.03 in Utah. The average factory price for the whole country in 1919 was $1.69, an increase of only 6 per cent. over 1918.

The exports of hydraulic cement from the United States in 1919 amounted to 2,463,689 barrels, valued at $7,516,019, or $3.05 per barrel, increases of about 9.27 and 16 per cent., respectively, over 1918.

THE INVESTIGATION OF FATIGUE PHENOMENA IN METALS

IN 1915 Mr. Ambrose Swasey gave a fund of several hundred thousand dollars, the income of which was to be used "for the advancement of arts and sciences connected with engineering and for the benefit of mankind." The income of this fund has been given in small amounts to various engineering investigations by the Engineering Foundation, which is the body organized to administer the fund. Last spring the governing board of the foundation decided that it would be advisable to give the bulk of the income for the support of one major research, and they asked the National Research Council to recommend some piece of research to be supported.

During the war the National Research Council had organized a committee to study the failure of crank shafts of airplane engines, of welded ship plates, and of other metal parts of machines under the repeated loads applied to them in service. The committee on fatigue phenomena in metals was

the title of the committee. Its chairman was Professor H. F. Moore, of the department of theoretical and applied mechanics, of the University of Illinois, and during the war and afterward some small pieces of research work were carried out under the auspices of the committee, mainly in the materials testing laboratory of the college of engineering of the University of Illinois by the chairman of the committee, and by W. J. Putnam, and A. G. Gehrig. The National Research Council recommended that the bulk of the income of the Engineering Foundation be given to the support of an extensive investigation of the resistance of metals to fatigue under repeated loading, and that Professor Moore be asked to take charge of the investigation.

Engineering Education, from which these facts are quoted, states that the formal arrangements have been completed for the active prosecution of this work, with headquarters and a laboratory at the University of Illinois. The financial support for the investigation will amount to $30,000, and it is expected to extend over a period of two years. Material for study and apparatus is already arriving, and a room is being fitted up for the installation of the score or more of special testing machines which will be required for the investigation.

The investigation is under the joint auspices of the Engineering Foundation, the University of Illinois Engineering Experiment Station, and the National Research Council, the last-named body being represented by an advisory committee of nine members, of which Professor Moore is chairman. In addition to the funds supplied by the Engineering Foundation, the university furnished Professor Moore's services, light, heat, power, a laboratory room, and the use of the standard testing equipment of the materials testing laboratory.

SCIENTIFIC NOTES AND NEWS

The degree of doctor of laws was conferred on Professor Theodore W. Richards, director of the Wolcott Gibbs Laboratory of Harvard University, at the University Day exercises of the University of Pennsylvania.

Professor Anton J. Carlson, chairman of the department of physiology at the University of Chicago, has been made an honorary M.D. by the University of Lund, Sweden. Professor Carlson has also been made a corresponding member of the French Biological Society.

Professor J. M. T. Finney, of the Johns Hopkins University, and Dr. Charles H. Mayo, of Rochester, Minn., have been elected honorary fellows of the Royal College of Surgeons of England.

Mr. Gifford Pinchot, of Milford, Pa., former chief forester of the United States, has been appointed commissioner of forestry of Pennsylvania by Governor Sproul to succeed Robert S. Conklin, of Columbia, who resigned to become a member of the State Water Supply Commission.

Dr. Henry Graves, chief of the U. S. Forest Service, and Albert F. Pottee, associate forester, have resigned.

Professor Robert B. Riggs, for thirty-three years professor of chemistry at Trinity College, will retire at the close of the present college year.

Mr. R. M. Brown, formerly librarian of the Coast and Geodetic Survey, has accepted an appointment with Rand, McNally and Company, to prepare and edit material for a new edition of their atlas of the world.

Mr. George A. Rankin, formerly with the Pittsburgh Plate Glass Company, and captain in the Chemical Warfare Service during the war, has joined the staff of the Geophysical Laboratory of the Carnegie Institution of Washington.

Julius Matz, formerly with the Florida Agricultural Experiment Station and for the past year assistant plant pathologist at the Insular Experiment Station of Porto Rico, has been appointed chief of the division of botany and plant pathology at the Insular Experiment Station, Rio Piedrus, P. R., beginning on January 1, 1920.

MR. PAUL A. MURPHY, field laboratory of plant pathology, Charlottetown, P. E. I., has resigned his position as officer in charge of potato disease investigation under the Dominion Department of Agriculture and will take up work on April 1, as assistant with Dr. Pethybridge in the division of seeds and plant diseases in charge of plant pathological work in Ireland. His new address will be Royal College of Science, Dublin.

ASSOCIATE CURATOR W. R. MAXON, of the U. S. National Museum, and his assistant, Mr. Killip, are making the Cinchona Tropical Botanical Station their base during March and April, while carrying on botanical exploration of the northern slopes of the Blue Mountains of Jamaica. Only small areas of this region have been actually explored by botanists, and it is to be expected that many interesting types of ferns and angiosperms are yet to be discovered in the primeval forest which covers this region.

WE learn from the *Journal* of the American Medical Association that Dr. Victor G. Heiser, of the Rockefeller Foundation, has returned to New York after a trip to Porto Rico with Dr. Grant to make a study of sanitary conditions of the island, especially as regards hookworm disease. Dr. Louis Shapiro, of the Rockefeller Institute, is now in Colombia at the request of the Colombian government, making a study of the prevalence of leprosy, malaria and hookworm disease.

THE Puget Sound Biological Station will hold its annual session, beginning June 21 and continuing for six weeks with the class work. The station, however, is open several weeks longer. The staff, exclusive of assistants, this year will consist of Dr. B. M. Allen, embryology, University of Kansas; Dr. Nathan Fasten, morphology, University of Washington; Dr. T. C. Frye, director, algæ, University of Washington; Professor F. W. Gail, algæ, University of Idaho; Dr. E. J. Lund, physiology, University of Minnesota; Dr. V. E. Shelford, ecology, University of Illinois, and Professor A. R. Sweetser, plant taxonomy, University of Oregon.

PROFESSOR ALBERT M. REESE, of West Virginia University, lectured upon "The Work of the Tropical Biological Station of British Guiana," with special reference to Crocodilia, on March 5, at Oberlin College.

PROFESSOR ARTHUR KEITH delivered the Galton lecture before the Engineers' Education Society on February 16, the anniversary of Sir Francis Galton's birth.

AT a public meeting held on March 7, at Oxford University, it was decided to form the "Osler Institute of General Pathology and Preventive Medicine" as a permanent memorial to the late Sir William Osler.

PROFESSOR OTTO BÜTSCHLI, of Heidelberg, distinguished for his contributions to cytology and other departments of experimental zoology, died early in February, aged seventy-two years.

UNIVERSITY AND EDUCATIONAL NEWS

THE General Education Board has appropriated $250,000 to an endowment fund of at least $500,000 to be used by Howard University for medical education, "the income from the appropriation to be made available pending the completion of the full amount."

PLANS have been completed for a new chemical laboratory at Cornell University, and work will start immediately upon the closing of the spring term. The increased facilities which the new laboratory will afford will enlarge the scope of the department and will make possible the opening of new branches, in particular a department of industrial research for chemists.

ACCORDING to plans now being considered by the authorities of the Johns Hopkins University, the libraries of the hospital, the school of hygiene, and the medical school ultimately will be collected under one roof in a new library building to be erected in the hospital group.

GIRTON COLLEGE, Cambridge, has received a gift of £10,000, the capital and interest of

which are to be applied during the next twenty years for the encouragement of scientific research by women in mathematical, physical and natural sciences.

Dr. G. Canby Robinson, dean of Washington University Medical School, St. Louis, has resigned to accept a position as dean and professor of medicine in Vanderbilt University, Nashville, Tenn.

Dr. Arthur M. Pardee, professor of chemistry at Washington and Jefferson College, has been appointed professor of chemistry and head of the department at the University of South Dakota to take effect next September.

The *British Medical Journal* states that in the appointment of professors to German universities precedence is at present being given to university teachers who have left towns which have passed out of Germany's possession. The anatomist, Professor Hugo Fuchs, who had recently been appointed to Königsberg, has thus been transferred to Göttingen as Merkel's successor.

DISCUSSION AND CORRESPONDENCE

IONIZATION AND RADIATION

Recently I came across a communication by Professor R. A. Houstoun[1] in which it was proposed to explain ionization of gases by X-rays on the basis of the classical conception of electrodynamics, by considering the intereference of spherical wavelets in which the phases are distributed at random. Professor Houstoun stated:

> When X-rays pass through a gas, only a very small fraction of the molecules—in favorable circumstances, one in a billion—is ionized by them, and the extent of this ionization is unaffected by temperature. Writers on radiation seem to have difficulty in reconciling this with the wave theory of light. I venture to suggest that the difficulty arises from an imperfect comprehension of what the wave theory requires.

After applying Rayleigh's solution of the problem of the phases at random to ionization, he arrived at the conclusion:

> Thus it is not necessary to assume that X-rays consist of neutral atoms, or that the ether has a fibrous structure, or to take refuge in the nebulous phraseology of the quantum theory; the explanation follows naturally from the principle of interference as expounded by Fresnel.

This explanation of ionization occurred to me some ten years ago but I had soon to abandon it because it led to results which are at variance with facts.

Let I/r^2 denote the intensity in a wavelet at a distance r from the source, and n be the number of wavelets coincident at that distance. Then the probability of a resultant intensity greater than J is given by

$$e^{-(Jr^2/nI)}.$$

Therefore if J equals the minimum intensity necessary to ionize the molecules of a gas, the number of molecules ionized is proportional to this expression. Thus on this theory the intensity of ionization of a gas falls off exponentially as its distance from the source of X-rays is increased—a result which is contrary to the experimental fact that the intensity of ionization varies inversely as the square of the distance.

H. M. Dadourian

Trinity College

HOW DID DARWIN WORK?

Last year Professor Francis B. Sumner published a very suggestive and interesting paper in *The Scientific Monthly* for March, dealing with "Some Perils which confront us as Scientists." In it he quoted with approval an indignant query: "Under what project did Darwin work?"—and again, "one wonders what institution or organization Newton or Darwin belong to." The solitary worker of Down seems the incarnation of scientific genius illuminating the world with the products of its own combustion. On closer inspection, however, this conception is seen to be illusory. In the whole history of science there has perhaps never been a man who worked more faithfully and persistently on a project. It was his own project to be sure; but none the less a definite project. So also,

[1] *Nature*, April 24, 1919.

there has rarely been a man who so constantly sought the cooperation of all who could and would render him assistance. The "Origin of Species" is full of acknowledgements to his friends and correspondents, without whom he would have been comparatively helpless. From a close study of Darwin's life, we arise with the conviction that it is precisely the man of genius who should be the center of a cooperating group, and that it is through such cooperation that human knowledge, at least in the biological sciences, is chiefly advanced. To-day the adequate study of even a simple species of plant, as I have found in dealing with *Helianthus tuberosus,* requires not only a general botanist, but a plant physiologist, a taxonomist, a chemist, a soil physicist, an entomologist and others. Who is so versatile that he can perform all these functions? Yet our institutions are so constituted that each department stands by itself, and cooperation is no part of the regular program. We must not permit ourselves to be dictated to by persons who can not understand our aims or the conditions under which we must work, but the state has a right to demand efficiency. Are we sure ourselves, and can we convince others, that we are not overdoing our individualism? The world needs to be made wise and honest: can we afford to refuse to work together to this end?

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

A CONVENIENT DEMONSTRATION MOUNTING FOR JELLYFISHES

THE writer has found the following method of mounting jellyfishes (Scyphozoa), both convenient and satisfactory besides permitting the observation of many structures usually only clearly seen when specimens are removed from the preserving jar.

Choose from the material on hand a jellyfish whose diameter is approximately that of a Petri dish in which it then may be placed, enough 4 per cent. formalin being added to cover the specimen. After the dish has been covered, it may be forced down in a mold of fresh plaster of Paris until the space between the upper and lower halves of it is sealed, and the top of the upper half is flush with the surface of the mold. When the mold has firmly set, any obscuring plaster of Paris may be scraped from the glass, or the mold itself suitably shaped up with a scalpel. Formalin solution condensing at any time on the upper lid may be displaced by manipulation.

Perhaps the most convenient molding frame is a paper box of a size adaptable to that of the Petri dish, although it may be of any shape. It is best to vaseline the interior of the box, in order that the hardened material may come away freely. With some care, a clean-cut looking mount may be secured. If desired, the plaster of Paris part may be given a coat of shellac, making it more durable from the laboratory standpoint. Data concerning the specimen may then be placed upon it with India ink.

It is seen that the above procedure is a modification of an old laboratory trick whereby odd bits of natural history specimens such as corals, sponges, specimens in vials, etc., may be given a convenient and useful mounting.

N. M. GRIER

HOLLINS, VA.

ORGANIZATION OF THE AMERICAN GEOPHYSICAL UNION

AT its meeting on June 24, 1919, the "American Section of the proposed International Geophysical Union" passed the following motion:

Moved: That the members of the Section who go to the Brussels meeting be constituted a committee, with power to add to its membership, to consider permanent organization of the Section—the committee, after completing a plan for such organization, to report to a meeting of the Section, to be called at the discretion of the acting chairman of the Section, for the purpose of perfecting the permanent organization. *Adopted.*

The Brussels meeting referred to is that which was held from July 18 to July 28, 1919, to organize the International Research Council, and International Unions affiliated with it.

At this conference the International Geo-

detic and Geophysical Union was formed, with six sections, as follows: (*a*) Geodesy, (*b*) Seismology, (*c*) Meteorology, (*d*) Terrestrial Magnetism and Electricity, (*e*) Physical Oceanography, and (*f*) Volcanology. Officers elected were listed in SCIENCE[1] of October 31, 1919.

The delegates who went on behalf of the geophysical sciences from the United States to these meetings at Brussels, were Messrs. William Bowie, Chairman, L. A. Bauer, G. W. Littlehales, and Rear-Admiral Edward Simpson. At Brussels Messrs. C. E. Mendenhall and H. S. Washington who were already abroad were added to this delegation.

At the call of the chairman of the "American Section," on October 31,1919, an informal conference of these delegates, constituting the committee on organization authorized on June 24, with other members of the "American Section" who reside in and near Washington, was held at the offices of the National Research Council. At this meeting, after a general exchange of views, a subcommittee or organization to draft proposals for statutes, was designated by the committee of delegates —to consist of Messrs. L. A. Bauer, Chairman, William Bowie, W. J. Humphreys, G. W. Littlehales, and H. O. Wood. This subcommittee held several meetings early in November, at some of which it had the benefit of further extended conference with Messrs. Mendenhall and Washington, who were present at Brussels. As an outcome, a draft of "Proposals for the Permanent Organization and Statutes of the American Geophysical Union" was drawn up, approved by the committee of delegates charged with the duty of preparing for permanent organization, and since it was not considered expedient to call a meeting of the section in Washington this draft was submitted for a vote by mail ballot to all members of the "American Section." An affirmative vote was returned by a considerable majority of the members prior to the date set for the count of ballots and subsequent affirmative ballots delayed in transit were received from nearly all members. There were no dissenting votes.

These statutes of the American Geophysical Union, thus approved by the "American Section," were then submitted to the executive board of the National Research Council and were approved by that body on December 20, 1919, and on February 14, 1920, the American Geophysical Union was made a Committee of the Executive Board

This action established the American Geophysical Union as a permanent organization superseding the "American Section of the proposed International Geophysical Union." As thus constituted the American Geophysical Union serves as "the American 'National Committee' of the International Geodetic and Geophysical Union, and as the Committee on Geophysics of the National Research Council." Its initial membership is the membership of the "American Section" as this stood on July 1, 1919, together with the Chairman of the Division of Physical Sciences, the Chairman of the Division of Chemistry and Chemical Technology, and the Chairman of the division of Geology and Geography of the National Research Council, and the American officers of the International Geodetic and Geophysical Union and of its sections, as members ex-officio. Its general administration is delegated to an Executive Committee made up of the chairman and secretary of the union, and the chairman of each of its sections which, initially, are the same as those in the International Union, viz: (*a*) geodesy, (*b*) seismology, (*c*) meteorology, (*d*) terrestrial magnetism and electricity, (*e*) physical oceanography, and (*f*) volcanology.

At its first, regular, annual meeting officers will be elected in accordance with the terms of the statutes. Meanwhile, by action of the "American Section" taken on June 24, 1919, the chairman and secretary of that organization continue to serve.

By action of the provisional executive committee of the "American Section" an election of acting chairmen for each of the newly constituted sections was held in January, 1920,

[1] Bauer, L. A., "Geophysics at the Brussels Meeting," July 18–28, 1919, SCIENCE, October 31, 1919, 1296, pp. 399–403.

by mail ballot counted on February 2, in order to constitute an acting executive committee conforming in organization with the statutes, to prepare the way for the first annual meeting. As a result of that election the following acting chairmen were elected: Section (*a*) William Bowie, Section (*b*) Harry Fielding Reid, Section (*c*) C. F. Marvin, Section (*d*) L. A. Bauer, Section (*e*) G. W. Littlehales, and Section (*f*) H. S. Washington.

HARRY O. WOOD,
Secretary, American Geophysical Union

SPECIAL ARTICLES

IS UNPALATABLE FOOD PROPERLY DIGESTED?[1]

IT is well known that different psychic stimuli promote or retard the secretion of digestive juices. The following experiment was conducted to determine whether the ultimate return to the body from unpalatable food was different from that of the same food palatably served.

The experimental procedure was simple. A 7-day period during which the subjects were on a uniform diet, served palatably and amid pleasant surroundings, was followed by a 2-day period during which the same diet was fed in an unpalatable condition and in dirty and unpleasant surroundings. The food was rendered unpalatable and unappetizing by the following treatment. All the food ordinarily used for each meal (meat, biscuits, jelly, cornstarch, pudding, oleomargarine, etc.) was stirred together in a large, flat porcelain dish. The dish itself was smeared with animal charcoal, as was the beaker used as a drinking glass. The table was dirty and strewn with dirty dishes. A little indol was sprinkled about under the table. The subjects were kept in ignorance of the constituents of the unpalatable mixture. The food was so unpalatable that one subject vomited his first meal shortly after he had eaten it.

The table shows the finding, on the other subject.

The differences in utilization of the palatable and unpalatable foods were quite small as were the variations in nitrogen retention. This short test indicates that flavor is not the outstanding dietetic asset that some people would have us believe. If the stomach and intestine can only be cajoled into making the proper effort, the unsavory concoction can be digested just about as satisfactorily as can the food mixture which makes a stronger appeal. If the things we eat have proper food value, we need not worry unduly as to their digestion, absorption, and utilization by the normal body. This ought to be good news to millions of people who eat unpalatable food in untidy surroundings, in spite of the fact that one of our leading physiologists says "What man likes best he digests best." This experiment simply shows how insulting we can be to the normal stomach and get away with it but does not necessarily prove this to be the wisest policy.

Period	No. of Days	Nitrogen							Percentage Utilization
		Ingested		Excreted			Balance		
		Daily, Grams	Period, Grams	Urine, Grams	Feces, Grams	Total, Grams	Period, Grams	Daily, Grams	
Palatable	7	10.75	75.25	62.95	10.06	73.01	+2.24	+0.32	86.7
Unpalatable....	2	10.75	21.50	17.03	3.09	20.12	+1.38	+0.63	85.7

RALPH C. HOLDER,
CLARENCE A. SMITH,
PHILIP B. HAWK
JEFFERSON MEDICAL COLLEGE,
PHILADELPHIA

[1] From the Laboratory of Physiological Chemistry, Jefferson Medical College, Philadelphia, Pa.

THE WESTERN SOCIETY OF NATURALISTS

THE Northwestern Division of the Western Society of Naturalists held its holiday meeting on January 2, in Portland, Oregon.

There were present delegates from the states of Oregon and Washington. The afternoon program was taken up with a discussion of "The Rôle of Research in the Development of Northwest Colleges" and also with a discussion of special papers. The evening program was given over to a symposium on premedical education. The following papers were read:

"The premedical education as a surgeon sees it," by Dr. Richard B. Dillehunt, of Portland.

"The premedical education as the medical school would like it," by Dr. H. B. Myers, University of Oregon Medical School.

"A premedical education and chemistry," by Dr. W. C. Morgan, of Reed College, Portland.

"A premedical education and biology," by Dr. H. B. Torrey, of Reed College, Portland.

"A premedical education as a university course," by Dr. J. F. Bovard, University of Oregon.

The papers were followed by a general discussion. At the business meeting Dr. G. B. Rigg, of the University of Washington, was elected Divisional Secretary for the ensuing year.

John F. Bovard,
Secretary

THE AMERICAN MATHEMATICAL SOCIETY

The two hundred and eighth regular meeting of the American Mathematical Society was held at Columbia University on Saturday, February 28, 1920, extending through the usual morning and afternoon sessions. The attendance included twenty-eight members. Vice-president R. G. D. Richardson occupied the chair. The following new members were elected: F. J. Burkett, Pennsylvania State College; A. D. Campbell, Cornell University; Y. R. Chao, Cornell University; R. E. Gilman, Brown University; D. C. Kazarinoff, University of Michigan; Norman Miller, Queen's University; G. M. Robison, Cornell University; Jung Sun, Pekin Academy; W. H. Wilson, State University of Iowa; S. D. Zeldin, Massachusetts Institute of Technology. Six applications for membership were received.

Professor Oswald Veblen, of Princeton University, was appointed to succeed Professor E. W. Brown, resigned, as representative of the society in the division of physics of the National Research Council.

Steps were taken to submit the question of the incorporation of the society to the vote of the members at the April meeting.

The following papers were read at this meeting:

Joseph Lipka: "On the general problem of dynamics."

A. R. Schweitzer: "On the iterative properties of the algebra of logic."

A. R. Schweitzer: "On improper pseudogroups, with application to the abstract field."

G. H. Hardy: "On the representation of numbers as sums of squares and in particular of five and seven."

J. W. Alexander: "On the representation of any n-dimensional two-sided manifold as a generalized Riemann surface."

J. W. Alexander: "On the equilibrium of a fluid mass at rest."

T. H. Gronwall: "Qualitative properties of the ballistic trajectory (second paper)."

T. H. Gronwall: "On the distortion in conformal mapping."

A. A. Bennett: "Fictitious matrix roots of the characteristic equation."

Pierre Boutroux: "On multiform functions defined by differential equations of the first order."

B. H. Camp: "The significance of a difference, and the value of a sample."

J. H. M. Wedderburn: "On division algebras."

Edward Kasner: "Geodesics of surfaces and higher manifolds."

The next meetings of the Society will be at Chicago, April 9–10; San Francisco, April 10, and New York, April 24. The summer meeting and colloquium of the society will be held at Chicago.

F. N. Cole,
Secretary

SCIENCE

A Weekly Journal devoted to the Advancement of Science, publishing the official notices and proceedings of the American Association for the Advancement of Science

Published every Friday by

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.
NEW YORK, N. Y.

Entered in the post-office at Lancaster, Pa., as second class matter